changes, but they contained bloodvessels and showed precisely the same coverings as a uterine ovum at the same stage of development. The sections prove: (1) That the ovum can be embedded in the ovarian stroma outside the Graafian follicle. (2) That it embeds itself just as does the ovum in the uterus in connective tissue rich in bloodvessels. (3) As has already been shown by C. V. Tussenbroeck and others, the uterine epithelium is not necessary for plasmodial formation; as this ovum lies in the ovarian stroma neither can the plasodium be derived from the follicular epithelium. The two cases combined once again demonstrate the nature of implantation. The process involved is essentially a destructive one; the primary necessity for the ovum is a nidus of connective tissue rich in bloodvessels. Formation of decidua is not essential; it is a defensive reaction against the destructive activities of the trophoblast, which it limits and controls.

Pressure Conditions within the Abdomen.—SMITH (*Amer. Jour. Obst.*, 1908, lviii, 242), who has conducted experiments to increase our knowledge of pressure conditions within the abdomen, concludes: (1) Atmosphere pressure within and without the abdomen is almost exactly balanced, any variations at any point being caused by factors to be enumerated: There is no special universal positive pressure which has so frequently been assumed. Physiological increase or decrease of volume is attended by insignificant or no changes in pressure—the balance is practically maintained. A marked increase of volume from pathological conditions often takes place without disturbing the balance; this varies in different subjects. (2) Hydrostatic pressure at any point within the abdomen varies with the position of the body and the depth of the superimposed organs. (3) Negative pressure at uppermost points is possible under certain conditions where the walls of such uppermost points are rigid. In the upper abdomen in the upright position a negative pressure may exist, which has more or less to do with the support of the viscera. (4) Respiration causes small waves of pressure. (5) Coughing, sneezing, defecation, labor, and many movements of the body cause a very marked increase of intra-abdominal pressure by contraction of the muscles of the abdominal walls. Such pressure is transmitted in all directions and without diminution to every part of the contents and interior of the abdomen.

OTOLOGY.

UNDER THE CHARGE OF

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The Relation of Chronic Progressive Labyrinthine Deafness to Ménière's Disease.—ALEXANDER and MANASSE (*Ztschr. f. Ohrenheilk.*, 1908, lv, p. 183). Under the heading, Menière's disease, are commonly included certain labyrinthine symptoms, when they occur suddenly and are of an apoplectiform character, and these are found, according to the observations of Alexander and Manasse, in the greater majority in cases in which there has been a disease of the ear of long standing evidencing itself in chronic progressive labyrinthine deafness. In this paper Alexander and Manasse cite eleven cases of patients who for several years had exhibited, on one or both sides, evidences of this chronic impairment of hearing of an advanced type, and who suddenly became the subjects of the typical Menière's complex of symptoms, increased tinnitus, dizziness, nausea, vomiting, and increased impairment of hearing, the attacks with this sequence of symptoms being subsequently repeated. In some of the cases there were marked changes in the middle ear, considerable retraction of the drumhead, and calcareous deposits, but in all cases the symptoms, as determined especially by the hearing tests, pointed to the labyrinth as having been implicated in the progressive impairment of hearing. The labyrinthine changes in these cases, as observed by Alexander, Bruehl, and Manasse, included atrophic degenerative changes in the nervous elements and new connective-tissue formations. In addition to these commonly observed changes Manasse found, in two cases, multiple hemorrhages in the course of the auditory nerve and in the labyrinth, and in one case numerous hemorrhages in the ligamentum spirale, a lesser number in the modiolus, an extensive number in the macula saccule, and congeries of hemorrhages in the nerve itself. The Menière sequence of symptoms in this case was undoubtedly the result of these numerous hemorrhages; more exact evidence was furnished by a second case of chronic progressive labyrinthine deafness, which had been under clinical observation and which came to autopsy but a few days after an attack characterized by Menière's complex.

From these observations Alexander and Manasse draw the following conclusions: (1) That in cases of nervous deafness attacks demonstrating Menière's complex of symptoms are comparatively common; (2) that in cases of degenerative labyrinth atrophy multiple hemorrhages in the labyrinth and in the auditory nerve are to be expected; and (3) that the access of these attacks is referable to the hemorrhages in question.

HYGIENE AND PUBLIC HEALTH.

UNDER THE CHARGE OF

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Endotoxins.—At the July, 1908, meeting of the German Society of Microbiology (*Centralblt. f. Bak.*, October 27, 1908) the so-called endotoxins and their relation to the true toxins were discussed, and the discussion developed considerable difference of opinion. Pfeiffer lead the discussion and quite properly claimed that his research on the cholera poison was the first work that quite clearly indicates that the bacterial cellular substance contains a poison. Pfeiffer holds that the poison is firmly bound in the bacterial cell; it cannot be extracted except by deep-reaching chemical changes; it is not a secretion in any proper sense, and it is not set free nor can it injuriously affect the animal until the bacterium not only ceases to live, but is broken down, and its component parts pass into the solution. Pfeiffer states that he has never held that the endotoxins may not possess antigenic properties; but he has stated and still holds that in cholera and typhoid fever there is no evidence that an antitoxin is or can be produced. There may be found antibodies, but not in conformity with the theory of Ehrlich. He has not gone so far as some of his stuednts have, as to assert that anti-endotoxins may not be found, but if ever found they will most probably be quite different from the antitoxins already known, such as those of diphtheria and tetanus. There is a distinct difference between anti-infectious and antitoxic sera, and the therapy of the future will probably be more greatly benefited by improvements in the former than by discoveries of the latter. The dead cells of cholera bacilli are distinctly poisonous to man and experimental animals. This has been demonstrated beyond any question, notwithstanding its early denial by Gruber. The same is true of the cellular substance of the typhoid bacillus, and Friedberger has shown that the intravenous injection of only one-twenty-fifth of a milligram of the dead tpyhoid bacillus may cause most marked and prolonged toxic symptoms. However, the toxicity of the pathogenic bacteria is widely variable. Along with exquisitely toxic species are those that are relatively harmless; moreover, the susceptibility of animals to the endotoxins is also widely variable. Ransom and Metchnikoff found a poisonous body in old bouillon cultures of the cholera bacillus, and attempted to produce an antitoxin with it, but it is worthy of note that this poison does not appear in recognizable quantity until the culture has grown old and the bacilli undergo autolytic changes. Indeed, as Hahn has shown, fresh cultures of this bacillus are free from poison, consequently there seems to be no justification in holding or even suspecting that the cholera poison is a secretion. So long as the cell is growing and multiplying it forms no poison. The poison is not a secretion; it is a product of the dissolution of the cell. It is true that some atypical cholera strains, such as that of El Tor, produce an extracellular poison, but this is not true of typical cholera bacilli. Such a poison is not found either in tube cultures or in the peritoneal fluid of animals dead from the effects of these bacteria. Moreover, the poison that does result from the autolysis of old cultures varies in some properties at least, according to the stage in the decay and disintegration of the cells when it is examined. This is easily understood since it is more than probable that the primary products of autolysis undergo farther cleavage as time progresses. There have been several reports of the finding of soluble poisons in typhoid cultures, but in all of these the culture is old, and the poison is supposed to result from autolysis; besides such poisons are not highly active and by no means camparable with the toxin elaborated by the diphtheria bacillus. The same is true of the dysentery bacillus; even one-twentieth of a milligram of the dead cellular substance may kill a rabbit when injected intravenously, while Conradi found that 15 c.c. of the filtrate from cultures of from seven to thirty days old were without effect. It is true that Todd and Rosenthal obtained from filtered cultures of the dysentery bacillus, from twenty to thirty days old, a filtrate 0.1 c.c. of which killed rabbits when injected intravenously, but it should be remembered that growth stopped in these cultures at five days, at which time the filtrate showed no toxic action; therefore, the most reasonable conclusions is that the poison found in solution in such cultures from fifteen to twenty-five days later results from cellular autolysis. If the poison were a secretion it should appear while the bacteria are still alive and multiplying. It is hardly conceivable that dead or even inactive bacteria pour out secretions. Secretions are products of life processes. It is possible that some varieties of the dysentery bacillus elaborate soluble poisons, but these are the exceptions and not the rule; besides, the toxicity of the cell substance is always many times that of the filtered culture. Cholera, typhoid fever, and dysentery are infections and not intoxications.

Pfeiffer describes MacFayden's method of extracting the endotoxins, and quite properly rejects it as costly, uncertain, and by no means convincing. It will be remembered that MacFayden froze his typhoid bacilli at the temperature of liquid air, powdered them, and then extracted with 0.1 potassium hydroxide. The freezing probably gave him a fine powder and the dilute alkali extracted small quantities of the endotoxin. He might have omitted his costly freezing, and used a stronger alkali in proper menstruum, and then he would have secured a much more active poison. Boreger takes the bacteria from agar slants and suspends them in physiological salt solution. This is evaporated to dryness and finely rubbed in agate mortars, then this powder is diluted with water until the strength of physiological salt solution is again reached, then it is centrifuged, when it is found that the supernatant fluid is poisonous. By this means a fine suspension of the cellular substance is obtained, nothing more. Pfeiffer states that he has tested the so-called antitoxic cholera and typhoid sera of Kraus, but with only negative results. He concludes his part in this discussion as follows: (1) Endotoxins really exist and are important factors in the infections. The possibility of preparing anti-endotoxic sera is still an open question. (2) The soluble poisons found in certain old cultures are autolytic products and may have no concern in the disease processes. (3) There are other methods of obtaining poisonous bodies from bacterial cells. (4) Together with the endotoxins and their derivatives there are produced by some bacteria true toxins that are secretory products.

Kraus, Pfeiffer's most prominent opponent, replied and in concluding made the following statement: He believes that the cholera, typhoid, dysentery, pest, and meningitis organisms contain specific toxins which have antigenic properties, producing their specific antitoxins by which they are neutralized. These poisons can be obtained not only by extracting the bacterial cellular substance, but in the filtrates of bouillon cultures. Whether these poisons are secretions, as some authorities hold, or pass into solution from the disintegration of the cells, has not been determined, and should not be brought into the discussion because it is a matter of secondary importance, and its solution would not add much of value to what we know. The important thing is that the intracellular and the soluble poison are identical and produce antibodies and are neutralized by antitoxins. It is desirable that it be ascertained what the relation is between these toxins and the aggressins of Bail and the phenomena of anaphylaxis. Kraus thinks that the aggressins are poisonous and that the toxins are not concerned in the phenomena of anaphylaxis, both of which opinions are probably wrong.

Kolle and his students, one of whom, Heller, participated in the discussion, believe that the soluble and the intracellular toxin are different bodies, and this seems to be best warranted by the facts. Heller using Bacillus dysenteriæ of Shiga produced the specific symptoms and characteristic lesions of the disease in rabbits and mice by, (1) the filtrate from twenty-day cultures, (2) filtered washings from living cultures, and (3) the dead washed bacillus. Then he attempted to obtain antitoxic sera with each of those and succeeded, but he found the antitoxic serum obtained with the dead washed bacillus was much less active than the others, and moreover, that the other sera protected against the extract and filtrate better than against the dead bacillus. He concludes from this that the toxin and endotoxin are not the same, but that the dead washed bacillus still contains some of the true toxin.

It may be seen from the foregoing abstract that our German friends are divided into at least three groups in their ideas concerning the relation between the true or soluble toxins and the so-called endotoxins. Pfeiffer holds that some bacteria, among which are those of cholera, typhoid fever, and dysentery, elaborate no true toxins, but that their poisons are intracellular and when found in solution they result from cellular autolysis or disintegration, and that active anti-endotoxic sera have not been produced. Kraus holds that these same pathogenic bacteria, like those of diphtheria and tetanus, do elaborate true toxins with which specific antitoxic sera may be prepared, and he does not care whether we believe that these toxins are secretions of the living cells or result from the breaking up of the cellular substance. Kolle and his students hold that these same bacteria have both true toxins and endotoxins, and that antibodies to the former are not produced by nor do they neutralize the latter. All, however, seem to agree that the poison elaborated by each species of bacillus is specific. [The writer of this review holds that the true or soluble toxins, like those of diphtheria, tetanus, botulism, etc., are specific and produce specific antitoxic sera, but that the endotoxins of the cholera, typhoid, and tubercle bacilli are not specific, but that every true protein, whether it be bacterial, vegetable, or animal, contains a poisonous group which in its effects on animals is the same. The presence of a special protein in the blood of an animal develops a specific enzyme which splits up that special protein and liberates the poisonous group to the detriment of the health and possibly at the cost of the life of the host.—V. C. V.]

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